TECHNICAL EDUCATION

ON Saturday last a meeting was held at the rooms of the Society of Arts, of scientific men and others interested in the promotion of technical education among the working classes, and especially among the skilled artisans of this country. The chair was taken by H.R.H. Prince Arthur, who showed in the remarks which he addressed to the meeting an admirable appreciation of the extreme value of the subject to the future prosperity of the country. After the Secretary of the Society of Arts had explained the proposal of Captain Donnelly that the Society should establish technological examinations for the purpose of testing the practical knowledge and skill required in the application of the scientific principles involved in each art and manufacture, H.R.H. said:—

"The Society of Arts are endeavouring to encourage among those who are practically employed in various industries the study of art, and an accurate knowledge of its application in each branch of manufacture. The Society will not, however, attempt to teach the practice of those arts. Their object is rather to lay a sound foundation of all the principles on which those practices may be carried out to the best advantage. The Society wish and desire to give certificates, prizes, and scholarships to those who show that to practical skill as workmen they have added an accurate knowledge of natural principles in those matters. I am certain the necessity and advantage of combining scientific principles with practice will be seen by all. I only wonder that we are but just beginning to undertake this task. The machinery for these technological examinations already exists, and, in fact, the skeleton of industrial universities is ready to our hands. What we now require is funds to clothe it with and give it life, and to enable it to carry out its work—that is, to obtain sufficient prizes to encourage and reward the deserving youth of this country. If others would only do what Sir Joseph Whitworth has done by his noble endowment for mechanical engineering, we might soon hope to see our most sanguine expectations realised."

Little by little the country is gradually waking up to understanding the principles on which alone its future greatness as a manufacturing nation can rest. Many will probably echo H.R.H.'s wonder that "we are but just beginning to undertake this task."

VOLCANOES OF CENTRAL FRANCE

THE conduct of the war against the Goths having brought Procopius into Campania, he is thus led to give an account of Vesuvius. In the text before me the mountain is called Bebios, and ash is said to have fallen at Byzantium; but undoubtedly Vesuvius is meant, and there seem to be good reasons for suspecting that "Byzantium" should be "Beneventum." Both Besuios (= Vesuios) and Beneventum could be so written as easily to be mistaken for the names now in the text. I cannot indeed pretend to say when, I must leave that to the critics.

Procopius, "De Bello Gothico," l. ii. c. 4: "At that time Mount Vesuvius was roaring, but there was no eruption; however, in consequence of the roaring, an eruption was confidently expected and the surrounding population were in great fear. This mountain is about seventy stadii from Naples, bearing northwards from that place (*τετράμμ-νον αὐτῇ πρὸς Βορρᾶν ἄνεμον*). [The real bearing is about 10° S. of E. Is he confounding Naples with Stabiæ, where Pliny died?]* It is naturally escarped,—wide-spreading below, but the top steep and very rugged; and on the summit of Vesuvius, in the centre, there is a cavern, which appears to be remarkably deep, and it would seem as if it extended quite to the extremities (*ἔσχατα*) of the mountain; and any one who is bold enough to look down may see fire in it. It is of no consequence to the inhabitants when no flame is seen above it; but when the mountain sends forth an explosive roaring it generally happens that not long afterwards a vast quantity of dust is thrown up, and if this destructive shower meets a traveller on the road he can by no means escape with his life; and if it descends on houses, they fall, crushed down by the mass of ash; and if a violent wind happens to be blowing, the ash is carried to a great height, out of people's sight, and is transported wherever the wind may carry it, and it may fall in some country at a very great distance. And they say that formerly, when it fell in Byzantium [*qu.* Beneventum?], the people there were so alarmed that they have made it a rule (*ἔγνωσαν*) in general assembly from that day to this to propitiate the deity by yearly supplications (*λιταῖς*). At another time (they say) it fell in Tripolis in Lybia. And they say that this roaring first (?) took place a hundred or more years ago, and that latterly it occurred much more frequently, but with decreasing violence.* They say, too, that when Vesuvius happens to eject this dust, the country where it falls is sure to produce abundant crops. On this mountain especially, beyond all others, the air is very light, and highly conducive to health, and time out of mind medical men have unhesitatingly sent thither their consumptive patients. Such, in the main, are the circumstances in connection with Vesuvius."

Vesuvius lies about 40° 47′ N. by 14° 26′ E., Byzantium about 41° N. by 28° 59′ E.; so a difference of 14° 33′ of longitude on the parallel of 41°, or, by a rough estimate, a distance of 750 miles. Is it possible that ashes ever fell at that distance in such quantities as to terrify the people, and drive them to institute Rogations? Is it likely that a professional historiographer, a resident in the very town where the alarm is said to have occurred, and where the Rogations had been instituted and constantly observed, could have first heard of these facts in Italy and from Italians? If this is scarcely credible, it suggests that we should read Beneventum. It is about thirty miles from the mountain—a distance not so great but that such events might have occurred there. He who wishes to have a vivid picture of them, let him read Pliny's second letter to Tacitus (vi. 20) describing what happened to himself at Misenum. The erroneous reading is somewhat countenanced by the fact that the statement seems intended to be an instance of great distance; whereas probably the heavy fall of ash and the consequent Rogations alone caused Beneventum to be mentioned, and Tripolis was the only case of distance.

While, therefore, the geologists are searching for ash at Vienne, the archæologists might oblige us by searching the annals of the principal Sees round Vesuvius, especially Beneventum. There were also Neapolis, Surrentum, Salernum, and perhaps other Sees, which our bishop might have held, but the scattered rays of light seem best to converge on Beneventum. At a much later date it is described as an Imperial vicariat; did it bear any such relation to the empire in the middle of the fifth century? Did it possess walls, a spacious forum, and a resident nobility? Could people have seen, or have fancied they saw, deer taking refuge in the forum? Is there any record

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* It has occurred to me to suggest πρὸς Βάριον ὅρμον for πρὸς Βορρᾶν ἄνεμον This would not be an unnatural reference in the secretary of a Byzantine general, if the Greek emperors already used that port for military purposes.

* My translation of this passage is open to criticism. I have even ventured to read ἀπισχυρισάμενον for ἀπισχυρισάμενοι, and I give an unusual sense to the word, by analogy to ἀποπλουτέω, ἀπομανθάνω, ἀποκραιπαλίζομαι, &c. The context seems to me imperatively to demand it. The Latin translation renders the text, as it stands, thus:—"According to them it is now a hundred years or more since the prior roaring occurred; the memory of the other is much more recent; moreover they affirm that it cannot but be, that, &c." Can θᾶσσον be translated by "more recent"? and why such violent asseveration about a simple agricultural fact? The difference does not affect our question; but it is of some importance in its bearing on the History of Vesuvius.

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or mention of a Bishop Mamertus, or of some intrusive bishop who may have been Mamertus? Labbe's "Sacrosancta Concilia" (at the British Museum or the London Library) might be consulted to see whether any hint is given of Mamertus having been in Italy, either in the letters of Leo, or (more probably) in those of his successor, Hilary (iv. pp. 1032—1047).

Procopius got his information during the first Gothic war (A.D. 535—540); the second lasted from A.D. 544 to A.D. 548. He died about A.D. 565, at the age of 60. Ought the "century or more ago" to be computed from the date of his information, or from the time when he wrote? This might make a difference of 10 or 20 years. If the former, then the commencement (?) and violence of the eruptions are thrown back to the early part of the fifth century. In any case they must have preceded rather than followed the interval from 455 to 463. Where did Prof. Daubeny get his date of A.D. 472? Certainly not here.

It is therefore very likely that about A.D. 455 Vesuvius may have been in a state of violent activity. In that year Rome was sacked. These are precisely the conditions which the hypothesis requires. Procopius was told that the Rogations were occasioned by such eruptions at about that date. If, then, Mamertus was the sole (?) author of them, he must have been present where the Terrors occurred. Is there positive evidence of the fact? If no positive evidence, for or against, can be found, it will then be time enough to go into other questions of probability. Here for the present I conclude.

HENRY NORTON

WATER ANALYSIS

II.

IN the last article* we assumed that the object of estimating the organic carbon and nitrogen has been fully understood by the reader; but in order to render all chance of our meaning being misunderstood in the remarks which are to follow, it will be as well to briefly capitulate the reasons which render these determinations so valuable. Carbon and nitrogen form with hydrogen the principal constituents of all organised bodies, and hence are found in all the excreta of animals. Nitrogen is found in animals to a much greater extent than in plants, while in the inorganic world this latter element is scarcely found at all, and carbon only in the form of carbon, coal or carbonic acid, none of which bodies are likely to be found in water except the last, the first two being insoluble. It hence follows that if a large quantity, or indeed any quantity, of carbon and nitrogen be found in water, their source can only have been organic in its nature, and if the proportion of nitrogen to carbon be more than one to five, the source is almost absolutely certain to have been animal.

Accordingly, when we find these two elements existing in waters in the proportions just indicated, we are justified in assuming the presence of some form of animal or organic contamination in them; and of all forms in which this contamination can exist, sewage is the most probable. It will thus be seen that when chemists assert that a water is contaminated with sewage, they do so on grounds the truth of which is easily demonstrable. That such waters containing effete animal matters are injurious, no arguments of ours will be required to prove; no persons of authority in sanitary matters have presumed to assert that such waters are harmless.

With regard to the method of obtaining evidence as to this contamination, however, considerable difference of opinion exists, and in the former paper we have endeavoured to show the worthlessness of the early processes of estimating organic matter by ignition of the residue, and by treatment with permanganate of potash; and though this last process is condemned by all, it is still in use.

We have also given reasons for regarding with distrust the results obtained by the use of Chapman, Wanklyn, and Smith's method of indirect determination of organic matter from the amount of ammonia evolved by the water, as we maintain that it cannot be shown that the ammonia evolved bears any distinct relation to the amount of organic matter present, and that with many waters it is difficult to obtain an accurate estimate of the ammonia thus evolved.

The process we have now to consider bases its claims to confidence on the fact that it gives an absolute determination of the quantity of carbon and nitrogen present in the water, and that it does do this can, we think, be proved without much difficulty.

The process is based upon the fact that when a body containing carbon and nitrogen is heated in contact with cupric oxide to bright redness, the carbon is converted into carbonic anhydride at the expense of a portion of the oxygen of the cupric oxide, while the nitrogen is liberated, partly in the free state and partly in the forms of its lower oxides, the quantity of these latter being reduced as much as possible by causing them to pass over the surface of red-hot metallic copper, which abstracts the oxygen and leaves the nitrogen free. In other words, the substances are obtained and estimated by what is known to chemists as a "combustion."

As far back as 1864 an unsuccessful attempt had been made by Weltzien to apply this process to the estimation of carbon in water. The failure was due to the very minute quantities available for estimation, and to the fact that the water was rendered acid with sulphuric acid before evaporation, a proceeding which directly tended to vitiate the results, even had no other obstacle intervened, as the acid gradually concentrated during evaporation, ultimately became sufficiently strong to char and decompose much of the organic matter present.

It was not until Dr. Hermann Sprengel had placed in the hands of chemists a new and powerful means of research in the shape of the admirable air-pump which bears his name, that it was possible to estimate directly the minute amounts of carbon and nitrogen which water, as a rule, contains.

In March 1868 Messrs. Frankland and Armstrong published a method of water analysis which they had elaborated, after eighteen months' work at the subject.

A quantity of water proportionate to the amount of ammonia found,* and varying from one litre in the case of a town water-supply, to 100 cubic centimetres in the case of a much polluted water, is introduced into a flask, and 15 cubic centimetres of a saturated solution of sulphurous anhydride are added, and the water boiled briskly for three minutes; the water is then removed from the source of heat, and a portion introduced into a hemispherical lipless glass dish of about 100 to 120 cubic centimetres capacity; this is placed on a steam bath, and to the first dishful two or three drops of a moderately strong solution of ferric chloride are added. Should the water leave but a small solid residue, or contain little calcic carbonate, a few drops of a solution of sodic sulphite should also be added. The dish is then covered with a cap of filtering paper stretched over a ring of cane, and the evaporation continued, the rest of the water being kept warm in the flask, and added from time to time to the dish. The laboratory in which this operation is performed should be kept free from dust, and no ammonia should ever be allowed in it.

The rationale of the process so far is very simple, and is as follows:—The first boiling with sulphurous acid expels the free carbonic acid in the water, and also any that

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* See NATURE, vol. vi. p. 104.

* If the ammonia be less than 0·1 part per 100,000, 1 litre should be used; if more than 1·0, a hundred cubic centimetres or less. (See Sutton's "Volumetric Analysis." J. A. Churchill, New Burlington Street, 2nd Edition, 1871, pp. 246-295.)

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